First reply to Mike Foster's review of the ACPD paper

" CLARA-A2: The second edition of the CM SAF cloud and radiation data record from 34 years of global AVHRR data"
by
Karl-Göran Karlsson et al.

Repeating general statement:

The article is generally well written and organized. The subject matter represents a great deal of work and a significant contribution to the cloud climate community.
That said there are ways I think the article could be improved. There is little discussion and few figures comparing CLARA-A1 to CLARA-A2, which I think makes it more difficult to understand the location and magnitude of improvements. I also think some of the changes could be described in more detail. Specific comments are below.

Reply:

We thank the reviewer for this positive evaluation. In the reply to the specific comments we will outline how we plan to better illustrate the changes and improvements of CLARA-A2 compared to CLARA-A1.

Repeating specific comment 1:

Section 4: There are lists of specific tasks performed for GAC data pre-processing, histogram, and albedo products. The cloud masking changes are less clear. A list of specific changes might help this. For example, P5 L22 states "Cloud detection during Polar day conditions over snow- and ice-covered surfaces has been optimized, and falsely-detected clouds during Polar night conditions have been largely removed." How was this done? It might also help to show specific examples – a comparison scene of cloud detection over semi-arid regions for CLARA-A1 and CLARA-A2 would be one possibility.

Reply:

- A list providing more details of the cloud masking algorithm changes will be added.
- For the specific question on cloud detection improvements in the polar regions we can say that we have systematically used CALIPSO cloud observations to identify when in particular falsely-detected clouds occur and with this information we have been able to reduce this problem considerably. For the polar night the focus has been on the latter rather than to improve polar cloud detection further since it is clear that AVHRR cloud detection capabilities during the polar night is already seriously limited. For daytime conditions the CALIPSO information has contributed to a better cloud discrimination over snow- and ice-covered surfaces. We will add this to the discussion in the text.
- Examples of changed results over semi-arid regions will be added.

Repeating specific comment 2:

Similarly there is little description of the changes to CTO retrieval. Would it be possible to include a little more detail as to what modifications were made to allow successful retrievals to jump from 70% to 97%?

Reply:

- We will add more details. Basically, the improvement has resulted from applying more physically sound constraints to the iterations (i.e., unphysical results are not accepted which allows more time to seek for physically reasonable solutions).

Repeating specific comment 3:

There is not a lot of discussion of how the changes compare to CLARA-A1 (other than Figure 2). I think it would be helpful to include CLARA-A1 data in a few of the comparison figures against PATMOS-x and MODIS (and maybe have a Hitrate panel for CLARA-A1 in Figure 3). Figures 7, 9 and 11 seem like good candidates for this.

Reply:

- We will consider adding more CLARA-A1 results, if possible. However, notice that no CLARA-A1 results have ever been produced for the period 2010-2015. Thus, since results in Figure 3 can only be visualised if having enough CALIPSO collocations (i.e., covering the full period 2006-2015) the specific request for that figure cannot be fulfilled. However, some inter-comparison results were produced for the period 2006-2009 and we can add this to the text (and a table).

- For Figures 7 and 11 we see no problem in also including CLARA-A1 results. Figure 9 needs some further consideration .

Repeating specific comment 4:

Section 7: The comparison against Norris et al. 2016 seems superficial, even by the preliminary standard defined in the manuscript. It is difficult to come to any conclusions based on the single figure 16. A linear regression to remove the ENSO signal might shed some light on this.

Reply:

- We agree that results do not allow firm conclusions, which we also wrote clearly (this should only be seen as a demonstration of what can be studied using the data record). However, to claim that result would be superficial is a too strong statement. We just repeated the study (or parts of the study) in the Norris et al. 2016 paper to see what happens if we add another 7 years to the 27 years that they studied. There are obviously differences but also similarities so it is clear that it is difficult to find very clear conclusions. However, the indication that the cloud changes seen by Norris et al., 2016 for mid- and high-latitudes are maybe not as clear in our study despite having added several years (which should give better prospects of finding a long-term trend) would actually call for further and deepened studies here. For this reason we still think this addition to the paper is interesting.

  The recommendation to remove the ENSO signal by a linear regression is questionable. Firstly, this was not done by Norris et al. 2016 in the original study and, secondly, a possible climate change signal could actually also mean a changed behaviour of the frequency and amplitude of ENSO signals. Thus, it would be dangerous to assume a static ENSO behaviour.

  In conclusion, we still think that this section adds something to the scientific discussion and that its presence could trigger deeper studies about specifically mentioned topics.

Repeating specific comment 5:

Figures 3 and 4 – the red-blue colorbar is usually used for temperature or something with positive and negative values. Also it is a little difficult to differentiate the value of Hitrate for higher values.

Reply:

We don't fully understand this comment. What we show is definitely something related to positive and negative values even if it expressed in a relative sense. Blue colours define poor validation scores and red values good validation scores. For example, in the case of Figure 4 the 50 % level of probability of detection must be considered as a critical negative case (i.e., here we only detect 50 % of all clouds). However, we admit that the choice of the intermediate point (i.e., when blue changes to red), which could be interpreted as the point where we go from bad to good results, is rather arbitrarily chosen. We will consider changing to a better color representation.

Repeating specific comment 6:

P4 – Are observations under twilight conditions excluded for all products, or just for the monthly averages?

Reply:

All cloud products except Cloud Physical Products (COT, REF, LWP, IWP) are based on all observations. The CPP exceptions are explained by the needed access to visible channel data for the retrieval methods.
The exclusion of twilight data is only applied for two additional sub-layers to the cloud amount product CFC. Thus, the main CFC product is based on all observations but in addition a user can choose to look also at a CFC sub-layer showing results exclusively at daytime or exclusively at nighttime. When defining these two sub-layers no data under twilight conditions was used. These sub-layers are available for both daily and monthly CFC products.

The description in the text is not correct and we will clarify.

Repeating specific comment 7:

P6 L7 – I don't understand this explanation. Is there perhaps a citation showing that the dry sub-tropical regions with decreased Hitrate are areas where sub-pixel scale clouds frequently occur?

Reply:

We are not certain what the problem is here (the reference to Page 6 Line 7 is not very specific). But if the question is only about the statement on the low Hitrate for dry-subtropical regions we can say the following:

Marine stratocumulus and cumulus clouds are dominant clouds over most marine ocean surfaces in the tropics and in the sub-tropics (if not being too close to the ITCZ). The frequency and the extent of clouds in these regions have definitely links to the size of the clouds. In the centre of sub-tropical anticyclones or highs cumulus clouds are mostly occurring as individual small-scale clouds (cumulus humilis +cumulus mediocris + cumulus congestus) with limited horizontal and vertical extent and with low frequency. Many of those cloud elements have sizes significantly smaller than the AVHRR GAC pixel (e.g., cumulus humilis or broken stratocumulus). However, away from the centre the number of clouds and their extent normally increases gradually with the distance from the centre. At some point the dominant cloud type may also change from individual cumulus clouds to stratocumulus clouds with larger horizontal extensions. The cloud distribution is also affected by ocean current effects so that regions with colder ocean surfaces may lead to almost overcast stratocumulus conditions. Good examples here are the ocean waters outside (to the west of) Namibia and Peru. What we claim here is that, since the occurrence of really small and exclusive (i.e., not accompanied by larger scale clouds) cumulus cloud elements is more likely for the reasons explained earlier in the central regions of the sub-tropical highs, the risk of encountering matchup problems between AVHRR GAC pixels (with 5 km dimensions) and CALIPSO observations (with 300 m width FOVs) is higher here than outside of the central portions of the sub-tropical highs. We think that this is supported by the pattern in the Hitrate plots which highlights the decrease in Hitrate over typical positions of the sub-tropical highs. In conclusion, we believe that the reduced scores over sub-tropical high regions must be related to a higher relative frequency of small (sub-pixel scale) cloud elements among all existing clouds leading to both enhanced CALIPSO collocation problems and to some extent also to a less efficient cloud detection.

The same thing could also happen over land areas with a high frequency of small-scale cloud elements but we think that the existence of more vigorous and widespread convection over land areas (as an effect of more heterogeneous surface conditions) might reduce this effect. However, we notice also low values over the eastern part of South-America and in eastern Africa which also could be linked to a high frequency of small cumulus cloudiness during periods of higher atmospheric stability.

To really prove this hypothesis is difficult (requires extensive high-resolution measurements over long periods) so we suggest that we modify the text in a way that we express this as a possible explanation rather than as a well-established truth. Maybe reliable global cloud-size statistics can eventually be collected from CALIPSO observations to reveal the answer, given that we could possibly be given a few more years of CALIPSO satellite operations.

Repeating specific comment 8:

Figure 12 – Hard to differentiate between blue and black dots

Reply: We will try to improve the visibility here.

Technical Corrections:

P2 L10 – 'lined out' should be 'outlined' P2 L18 – 'already' is unnecessary and can be removed P2 L37 – the grammar and use of semicolon in this sentence is odd – consider rewording P3 L1 – Sentence beginning with "Additionally, orbital drift..." is awkward. Consider rewording P3 L9 – incorrect usage of the word 'spurious' P5 L13 "is using" should be "uses" P5 L26 – Should be "spurious" or "false" cloud, not both.

Reply:

We will certainly correct this. Thanks for the suggestions.

FINAL REMARK:

A new version of this document will be made available after the manuscript is revised. The new version will verify exactly the changes which were made in the final manuscript.